ETENTION AFTER INTERVALS OF SLEEP AND OF WAKING

BY

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INTRODUCTION*

The results of the studies on the effect of sleep upon retention are of great theoretical and practical importance. If they are sustained the accepted theories of retention and of obliviscence will have to be considerably modified,[1] and the study-habit, frequently recommended,[2] of learning just before going to sleep will be empirically justified and rationally explained. The results have, furthermore, an important bearing upon the much discussed subject of retroactive inhibition.[3]

All the studies upon retention, even those dealing with the effect of sleep, have their origin in Ebbinghaus (7). When Ebbinghaus plotted his curve of retention he found a discrepancy between the empirical and the theoretical results of the 8.8 and 24-hr. periods that "was not credible" (see Table VIII, p. 42). The loss for the 24-hr. period from 24 to 48 hr. was almost three times as much as that for the 15-hr. period from 8.8 hr. to 24 hr. He suggested that this result might be due (1) to the fact that sleep, predominating more in the latter period, retarded the forgetting, or (2) to accidental and uncontrolled errors. After considering these possibilities he discarded the first and accepted the second.

Similar discrepancies (see Table VIII) were obtained in the curves of retention at the 8- and 24-hr. periods by Radosavljevich (24) and by Boreas[4] (2), and by Luh (16) for his relearning curve from the 4- to 24-hr. periods. Bean (1) and Finkenbinder[5] (8) attempted explanations of Ebbinghaus' and Radosavljevich's discrepancies at the 8- and 24-hr. periods. None of these investigators considered, however, the possibility that sleep might have effected the results by retarding forgetting. Foucault[6] (9) in a

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* From the Psychological Laboratory of Columbia University. This study was directed by Professor Karl M. Dallenbach.

1 Cf., for example the treatments of Hunter (13, p. 599–605) and McGeoch (18) and (19).

2 Cf., for example McDougall (17) has called attention to the fact that retroactive inhibition throws some light on the assertion of many persons that what is learned just before falling to sleep is remembered with unusual exactness.

3 For recent discussion of this subject see Skaggs (28), especially Chapters IV, V, VI, VII, and XII.

4 The writer is indebted to Professor R. S. Woodworth for the reference to and translation of Boreas' article.

5 Finkenbinder attempted a correction of Radosavljevich's data to smooth out the latter's curve. The writer, after a careful study of Radosavljevich's tables, is unable to verify Finkenbinder's correction.

6 The writer is indebted to Professor J. A. McGeoch for the reference to Foucault.

practically unknown paper reporting some work on retroactive inhibition, suggests, without giving Ebbinghaus credit for having thought of it, that the latter's discrepancy from 8.8- to 24-hrs. was due to the predominance of sleep which prevented the inhibition produced by day time activity.

The earliest investigator of this problem of sleep and retention was Heine (12), who apparently unaware of Ebbinghaus' suggestion, took her cue from a statement made by McDougall (17), see foot-note 2, page 5, our article. She compared retention after 24-hrs. when the nonsense syllables were learned just before going to bed and when they were learned with waking intervals interpolated between the learning and sleep. Four *S*s tested with the savings method gave reliable individual results definitely in favor of sleep, the percentages of retention being on the average 47% and 36%. A similar comparison with two *S*s, using the method of paired associates was likewise definitely in favor of sleep.

The first to study the problem in the light of the suggestion proposed and rejected by Ebbinghaus, were Jenkins and Dallenbach (14). These experimenters, using the method of retained members and testing the intervals of 1, 2, 4, and 8 hrs., discovered that for their two *S*s, on the average twice as many nonsense syllables were recalled after sleep than after waking. The superiority of the reproductions after sleep was greatest for the 8-hr. period. The results showed, however, a statistically reliable difference in favor of sleep at each of the other three time intervals. From the 2-hr. period on, the retention curves for sleep were practically horizontal lines. The results for the two *S*s were practically the same. Nicolai (23) studying reminiscence in tests of memory for objects, found the tendency at times, with adults, for the number of items reported to first increase at the 24-hr. interval. He thinks this substantiates Radosavljevich's recovery after sleep. Nicolai's was not an extended study of sleep and his results are only a suggestion. With children of a certain age he found reminiscence occurring first at 1 hr. rather than after a period of sleep. Dahl (5) using the recognition method, repeated the work of Jenkins and Dallenbach and reported group averages for 14 *S*s, showing that recognition of nonsense syllables and nonsense drawings was better after 4 and 8 hrs. of sleep than after the same time intervals of waking. After 1 and 2 hrs. the recognition after sleep was worse than after waking. This latter result appeared to be due in part at least to the greater drowsiness of the awakened

*S*s at 1 and 2 hrs. disposing them to make more yes answers. Spight (29), using 51 *S*s, compared two 12-hr. rest periods between the partial learning and complete learning of lists of paired words. One rest period occurred during the day and the other during the night, the major portion of the latter period being devoted to sleep. Comparing the average number of repetitions for complete learning under both conditions there was, between the group averages, a difference of half a repetition in favor of the night rest (PE diff. is 0.15), meaning that the learning with night rest took 9% less time.

The object of the present study was to obtain further information regarding the effect of sleep upon retention. It was proposed in particular to repeat Jenkins and Dallenbach's work, again using nonsense syllables, but this time employing the more precise and exacting method of savings; and it was hoped to obtain results, through refinement of technique, that would explain or reconcile the discrepancies obtained by the earlier investigations of Jenkins and Dallenbach, and of Dahl.

METHOD AND PROCEDURE

Subjects and Experimenters. The author (*V*) and his wife (*G*) acted alternately as *S* and *E*. Both worked without knowledge of their own results as *S*, and *G* without knowledge of any of the previous work on the specific problem being investigated and without having read any extended treatise on the experimental studies of memory.

There are, admittedly, some undesirable features in having each *S* act as the other's *E*, but the advantages outweighed the disadvantages; particularly as the disadvantages were partially if not entirely eliminated by the following procedure. Each constructed the other's syllable-lists which were so planned, as described below, that a given syllable did not appear in the lists of either oftener than once in 16 experimental-periods. The syllable-lists were not visible to *E* as they were being presented to *S*, and the hum of the motor used to drive the memory apparatus served as a sound-screen which masked the syllables from *E* as they were pronounced by *S*.[7] Furthermore, the records for every experimental-period were recorded upon a separate card, and the cards filed away until the close of the investigation. Not only did *S* not know his own results, but results of previous periods were also unknown to *E*, except as *E* might recall them from memory. Moreover, *S* did not know his experimental schedule, the experiments taking place according to planned haphazard orders known only to the *E*.

Memory Material. Nonsense syllables, selected from Glaze's (11) tables of 3-letter syllables,[8] were used as the memory material

[7] Both *E*s reported that they never clearly perceived the syllables as *S* pronounced them, and that they paid no attention to the individual syllables when they placed the lists on the memory apparatus.

[8] No account was taken of Glaze's association values. He spelled the syllables audibly as he presented them to the *S*s visually; he instructed his *S*s to search for meaning, that was their sole task; and he gave them a 3-sec. interval in which to do it. Therefore because of these great differences in procedure, task, instruction, and exposure-interval his association values would not seem to be comparable to the situation for the arousal of meaning as it exists in many memory experiments. Moreover, it would have been impossible to have followed his association values and to have constructed 630 lists according to the rules which were employed.

We did not wish to use a combination of 3- and 4-letter syllables as was done by Gamble (10). This would seem to make certain syllables more impressive than others. For the same reason we objected to 4-letter syllables of any form, even to the using of all 4-letter syllables, believing that various combinations of 3 consonants and a vowel or 2 consonants and 2 vowels are found to make some syllables more impressive than others. Moreover, it is

and constructed into 12-syllable lists. Syllables that had meaning in any language known to the *S*s[9] and that violated Gamble's (10) modification[10] of Müller and Schumann's rules, in so far as these rules applied to 3-letter syllables, were eliminated. The following variation from Gamble's rules should be noted here. (1) Since the material was limited strictly to three letter syllables no diphthongs were used as vowels. (2) We did not agree with Gamble's exclusion of -ĕr because of confusion with -ŭr or of -ŏr for confusion with -ȯr. In our system of pronunciation, see note 11, no confusion occurred. Accordingly syllables with such endings were not excluded in our selection. The remaining syllables were written upon cards, once with a short, and once with a long diacritical mark over the vowel. Using the vowels a, e, i, o, and u, this procedure gave us 10 vowel sounds for a list.[11] With this procedure, in some cases, one pronunciation of the vowel for a given combination of 3 letters would have meaning. The card with that pronunciation was discarded and the other retained. After the application of the above rules 1376 cards remained, 518 syllables with both long and short pronunciations and 340 syllables which could be used in only one pronunciation.

In the construction of the lists, Gamble's modification of Müller and Schumann's rules was again followed with the following modifications and additions. (1) Since we objected to using diphthongs (see note 8, p. 6) and had only 10 vowel sounds, the following rule was adopted. The vowel *sounds* may not be repeated in the first 10 syllables. Two vowel sounds of the same letter may never occur in consecutive syllables. In the 11th and 12th

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the writer's past experience that the pronunciation of 4-letter syllables as: nowf, homv, veyk, woik, etc., is extremely difficult.

[9] Both *S*s were native born Americans and had studied Latin and French. One *S* also had a reading knowledge of German. Any syllable was excluded which the *S*s recognized as literally a word or which when pronounced as the diacritical mark indicated, corresponded phonetically with a word, slang term, or nick-name known by the *S*s in any of the above mentioned languages. Both *S*s read through the syllables to determine this before the lists were constructed. As Gamble (10, p. 21–22) has pointed out, Ebbinghaus, and Müller and Schumann certainly did not have all the German words excluded from their lists. She has found 2.8% German words in 540 lists of the latter men and 26.8% words in similar English lists. Gamble made no effort to eliminate English words except where two successive syllables made a word or phrase. This same criticism applies to lists used by other investigators.

[10] DeCamp (6), Cason (3), and Luh (16) have also set forth modified rules for constructing nonsense syllables lists. But, in the writer's opinion, Gamble's modification of Müller and Schumann's rules is to be preferred.

[11] The following were the sounds: ā as in say; ă as in fat; ē as in be; ĕ as in bet; ī as in mine; ĭ as in it; ō as in no; ŏ as in mob; ū as in lute; ŭ as in up.

syllables a long vowel and a short vowel are to be repeated, any vowel letter except that of the 10th syllable. Of course the 11th and 12th vowels may not be the same letter. (2) The long and short markings (¯ and ˘ occurred in the various positions throughout the lists in a controlled haphazard order so that in the 630 lists a ¯ mark and a ˘ mark got no decided lead over the other in any given position and in the total lists occurred approximately the same number of times in each of the 12 positions. (3) No two or more successive syllables, taken literally or phonetically may obviously form a familiar word or phrase in any language known to the *S*.[12] Gamble's rules, with these modifications, are a great aid in pronunciation, and in avoidance of obvious inequality in the difficulty of syllables and lists when construction is left to chance.

In *regulating the occurrence of given syllables* the following procedure was adopted. The cards were thoroughly shaken in a good sized cardboard carton and drawn out as needed. If a syllable did not conform to the rules as above indicated it was put back in the box for future use. The cards used in the lists were put aside in packs numbered according to the lists which they composed and kept out of the box until too few cards were left available to construct lists according to the rules. The unused residue was then always worked in as soon as the rules permitted. The *E*s constructed 10 lists simultaneously, making the chances even as to whether a syllable would occur again, when its time came, in the lists of experimenter *G* or *V*. At the end of every 20 lists, 10 for each *E*, the lists were typed on strips of good quality bond paper, suitable to fit on the drum of the presentation apparatus. All three letters were small pica type, no capitals being used.[13] A special key on the typewriter gave the ¯ and ˘ marks for the vowels. If a wrong key was hit in typing a list, the list was retyped so that no erasures might effect the relative impression values of the separate syllables.

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[12] In spite of care, some violations of this rule are bound to be noticed by an *S*; and such was the case with us. Müller and Schumann excluded only those taken literally in German, not wanting to make the exclusion a subjective matter. Gamble refrained from excluding English words except where two together made a word or phrase. Our procedure seems to be an improvement.

[13] Some investigators have used all small letters; some have used all capitals; some do not say which were used. One at least used the first letter a capital and the other two small letters. It seems best to use all small letters. All capitals resemble too much the abbreviations of organizations or the letters of radio call stations. Initial capital and rest small would seem more likely to resemble a word.

It was possible to construct 90 lists, 45 for each *S*, without repeating a syllable. This left a residue of insufficient diversity to comply with the rules. In the building of the next lists this residue was used first where possible and when necessary was supplemented from the shuffled cards from the first 40 lists, 20 for each *S's* set. This residue of unused cards was kept separate from the re-shuffled cards until used. From the 110th list on, at the end of every 20 lists, 10 for each *S*, the earliest 20 then remaining from the previous lists (10 for each *S*) were shuffled into the supply for constructing lists. *Thus we can say that for the first 90 lists, 45 for each S, no syllable was repeated, and that from then on a syllable could not reoccur oftener than every 50 lists or every 16 experimental periods of the investigation.* In order to insure at least a minimum number of reoccurrences of a syllable, at the end of every 90 lists, 45 for each *S*, the residue of unused syllables was kept separate from the used lists being shuffled in. This residue was then worked into the lists just as soon as the rules permitted. A tabulation of when these residues were exhausted shows that *every syllable occurred at least five times* in the 630 lists constructed.[14]

The first 510 lists to be used in the main experiments were constructed during the month of July 1931 and put away to be forgotten. The remaining 120 lists were constructed during the last of October and the first part of November 1931.[15] Since the main experiment did not begin until December 11, 1931, it is obvious that at least three months intervened between the construction of a list and its use. Since more lists were used than had been planned, each *S*, when he had exhausted his set of 315 lists, started learning the set which he had constructed. Certainly no memory for these lists existed after having learned 315 others.

Apparatus. No suitable exposure apparatus being available,[16]

[14] Of necessity some syllables occurred more often; but, as mentioned above, never more often than every 50 lists, or 13 times in the 630 lists. Of course a syllable may have occurred in one *S*'s lists more often than in the other's. This was left to chance. We were interested in keeping both of us from hearing or seeing a syllable in any way—as either *E* or *S*—for the longest feasible period of time.

[15] It may be interesting to note the labor involved in this work. After becoming skilled at the task it still took 2½ hours to construct and type ten lists.

[16] The writer objects to using the hand operated memory drum as used by Luh (16) and others in the Chicago Laboratory, there certainly being a large variable error which might be a distraction to the *S*. The method is also a strain on the *E;* especially would this have been true in our case, where we wanted to present three lists at a sitting and at a faster rate than the 2 sec.

it was suggested to the writer[17] that an apparatus be constructed to be operated by a motor and to have the intermittent movement of the syllables as in the Lippmann apparatus.[18] Accordingly an apparatus was constructed consisting of two upright brass posts 15 cm. high and 4½ cm. apart, fastened at the bottom to a metal base of angle irons and joined at the top by a horizontal brass post. To the outside of one brass post was attached a stationary horizontal metal shaft upon which the memory drum could revolve. The memory drum consisted of a hollow aluminum cylinder of 51 mm. outside diameter and 67 mm. width, with one end open and the other closed, with the exception of a hollow tube extending in on the axis of the cylinder from the end and acting as the bearing on which the cylinder revolved when pushed onto the stationary shaft mentioned above. The hollow tube in the center of the cylinder contained a spring tongue which caught in a groove in the end of the stationary shaft, thus holding the revolving cylinder on the shaft. At the proper distance below the stationary shaft a revolving steel shaft went through both brass posts. The shaft fitted tightly so that there was no need for any additional bearings, the steel shaft revolving against the brass of the posts seeming to be satisfactory. On the cylinder side of the brass posts the revolving shaft contained a shoulder or collar containing two pins which engaged a cogwheel attached to the "closed" end of the cylinder or drum, thus producing an intermittent revolution of the latter as the shaft rotated. The other end of the revolving shaft contained a pulley to receive the motive power. Both pins in the

interval used in the various studies at the Chicago laboratory. The Ranschburg memory apparatus has been criticized by Schulze (26, 235–238) for the disturbing intermittent noise it makes while in motion. Neither Ranschburg's apparatus nor Wirth's modification (26) of it would seem to be suitable for our purposes where we want a specified interval of very short duration between each repetition of our list of 12 syllables. Schulze has also pointed out that the clock work kymograph as used by Müller and Schumann and by Meumann has the disadvantage that the exposed objects are continually in motion and when being used at high speeds cases of dizziness have been noticed. *Any* apparatus of the spring driven type may be open to two criticisms: (1) the speed may change as the tension on the spring becomes less, especially when the apparatus is very nearly run down; (2) individual experiments may be spoiled by the *E* failing to wind the apparatus.

[17] The writer is indebted to Mr. H. J. P. Schubert, then laboratory assistant in the Department of Psychology at Columbia University, for suggesting the idea of substituting a motor driven apparatus for the Lippmann apparatus or some similar arrangement.

[18] The Lippmann apparatus, manufactured by Max Marx, Mechanische Werkstätten, Berlin, is a spring driven apparatus which gives intermittent movement to the drum containing the lists. It is open to the same criticisms as any spring driven apparatus. (See above.)

shoulder or collar were held out by springs and had little levers attached so that either pin could be taken out of service by pushing the lever back and into a notch. Thus by taking one pin out of service the speed of the drum could be cut in half while the motive power remained constant. These are the essentials of the intermittent movement of the Lippmann apparatus; however, in it the motive power is a spring.

In our apparatus the motive power was obtained by using a General Electric Company experimental motor, a contact regulated, D.C., shunt wound motor, model No. 36062, Frame 325, H.P. not given (approximately 1/20 H.P.) which after careful testing was deemed accurate enough for our purposes.[19] It might be mentioned that quite a little difficulty was encountered in getting a motor which would maintain a "constant" speed on direct current, when started from cold and run for half an hour operating this apparatus. Finally this contact regulated motor was obtained.[20] A belt from the motor operated a 20–1 reducing gear,[21] which in turn operated by a belt, the pulley on the revolving shaft of the exposure apparatus proper. By a proper use of pulleys and an adjustment of the contact regulator of the motor, the desired speed of the exposure drum was obtained. The motor was mounted on a felt pad and then screwed to the board on which the apparatus was

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[19] The speed of the memory drum, as operated by this motor, was timed by an electrically controlled 1/100 second stopwatch, timing every other revolution. On six different occasions the timing was taken as above for periods of one half hour from cold. The speed of the drum at 19.5 seconds/revolution showed a variable error which had a range never over 1/10 second/revolution. During this time there was a slight increase of speed which ranged, on the six tests, from .2 of 1% to .39 of 1%. Such a small variable error and constant error should not affect the results of this investigation.

[20] No A.C. current was available at the apartment or the difficulty might have been solved more readily. Fractional horse power series wound D.C. motors cannot be depended upon for any constancy of speed when starting from cold and running for half an hour. Even a good quality shunt wound motor of the 1/20 H.P. type gave an increase of speed of the drum of .7 of 1% to 1.5%. The writer is indebted to the General Electric Company, and especially to Mr. F. A. Fredickson and Mr. F. H. Kuhn of the New York Fractional H.P. Sales Department of this company for extending to him the use of the above mentioned regulated motor. The general principle of the contact regulated motor is that of a centrifugal governor which, as the motor speeds up, makes an electrical contact, throwing more resistance into the motor and slowing it down slightly. As the speed decreases the contact is broken and the motor speeds up again. By proper selection of governor and external resistance a rather constant speed can be obtained within the limits of a small variation, even though the voltage may vary somewhat.

[21] It was found best to use very thin grease in the reducing gear; otherwise a change in the viscosity of the grease seemed to allow a change of the load on the motor and bring about a speeding up effect. Grease such as is used in free wheeling mechanisms of autos was found to be best to avoid this.

mounted, the screws being kept from direct contact with the motor base by thick felt washers, topped by metal washers. All this precaution was necessary because there was found to be considerable vibration from the motor, probably due to the centrifugal contact regulator mechanism.

In front of the memory drum and between it and the *S* was erected a vertical screen of neutral gray cardboard 30 inches high and 18 inches wide. An aperture was cut in this screen at the place where the syllables were to be observed, the size of the aperture being such that only one syllable was visible at a time. The *S* sat at a distance from which he could read the syllables comfortably, the height of the aperture being somewhat below a horizontal line from the eyes of the *S*, so that his head was held in about the usual angle in which reading is done. A small tubular electric lamp, such as is sometimes used over sewing machines or radio dials, illuminated the aperture from the *S*'s side of the screen. In addition to the vertical screen in front of the *S*, two sides or wings of the same material were attached to it, each wing being 20 inches wide and 30 inches high, and making inside angles of 110 degrees with the front screen. The ledge which supported the above lamp was also covered with gray cardboard. This three sided screen shut off all vision of the apparatus and the *E*. Black opaque screens were used at the window of the room, making it practically a dark room. An ordinary desk lamp was used on the *E*'s side of the table. The illumination on the *S*'s side of the screen was practically all due to the tubular lamp mentioned above. As long as this lamp was on, the *S* could scarcely tell whether *E*'s lamp was burning or not. Within the limits of variation in the commercial current and in the luminosity of the lamp filaments the illumination was a constant throughout the experiment. The three sided screen effectually concentrated the source of the *S*'s visual stimulation at the aperture where the typed syllables appeared upon the white paper.

Electric connections. A fiber disk attached to the side of the cog-wheel of the memory drum contained a copper pin that made contact with a spring copper contact once every revolution of the drum, thus operating a No. 22407 Stoelting electric counter. The fiber disc, being movable, was so adjusted that the contact was made just as the last syllable of the list started to leave the aperture.—The drum was marked so that the lists were easily placed at the same position on the drum each time, the lists being held in

place by rubber bands not visible through the aperture.—On a ledge by the *S*'s left knee, and easily accessible by a very slight movement of the arm and hand was a contact key which closed two circuits simultaneously. One circuit lighted a two volt lamp close to the *E* and the other circuit started an electrically operated 1/5 second stop-watch. *E* always gave the *S* a verbal "ready" signal and then in a second or two turned the motor switch, which made a loud click and served as a "get set" signal. The *S* always pressed his key as soon as the asterisks started to move and pressed it again as soon as the asterisks appeared at the end of the repetition on which he had made the correct anticipation. (Meaning of asterisks, see p. 16). Thus a fairly accurate measure of the elapsed learning time was obtained for each list; and served as a check on the reading of the counter and vice-versa, as well as a fairly accurate check on the consistency of the speed of the drum. Another contact key in the watch circuit on the *E*'s side of the screen made it possible for him to set the clock back to zero. The watch control was also wired so that by means of switches it could be connected to the copper contact of the memory drum, as it might be desirable to check the apparatus between experimental periods. The two volt lamp mentioned above served the purpose of an insistent stimulus to signal the *E* to stop the motor at the conclusion of the learning and to start the manually operated 1/5 second stop-watch used in timing the interval between lists. This light signal for the *E* was necessary because of the noise of the motor which made it easy to confuse the sound of the counter and the clock and because of the desire to have the timing of the interval start at once. The lighting of the lamp at the beginning of the learning served very little purpose except to show that the *S* was all set and going on with the experiment.

The sound screen. It was a stroke of fortune that in our search for a constant speed motor we came upon the one which we finally used. The noise which it made proved to be very desirable as a sound screen which excluded all noises but very unusual ones.[22] A similar device has been urged by various writers and employed in various studies in other fields.[23] Since this noise of the motor and

[22] Practice experiments had shown that the *S*s were very susceptible to distraction from any unusual noise. The writer heartily recommends the use of such a device in memory experiments with practiced *S*s, especially when conducted in city buildings near busy streets, where it is almost impossible to construct a sound proof room.

[23] Kellogg's (15) use of it in connection with his study of discrimination of sound intensities is one example.

the click of the counter at the end of every revolution of the drum are constants in the experiment they do not affect the comparison of sleep and waking.[24]

Method of learning—Instructions to S. The anticipation form of the learning method was used. The *S* read the first presentation audibly; and then attempted audible anticipation on each successive presentation of the list until the criterion of learning was attained. The thirteenth space on the list was taken up by three asterisks instead of the usual letters. The revolution of the drum was always started at this point. (See page 15.) On the second presentation, and successive presentations, the appearance of the asterisks served as the cue for the anticipation of the first syllable, the first syllable serving as the cue for the second, etc.[25] When anticipation of a syllable failed the syllable was read audibly as it appeared; and if the next syllable were known, its anticipation was crowded in before it appeared or partially before it appeared. *In this way all the active recalling that was possible was employed in the learning.* The *S* was instructed to do the learning without the use of any mnemonic devices, not to make an effort to look for meanings, and to refrain from verbal reviews if the opportunity should offer.[26]

The *criterion of learning* employed was one correct anticipation

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[24] The question may arise as to whether they affect the results so far as the obtaining of the absolute amount of retention is concerned. The work of Cassell and Dallenbach (4) would suggest that a *continuous* distraction such as the noise of our motor if it is distracting at all, is less distracting than other types of auditory stimulation. They suggest the further hypothesis that the effect of a distraction, in addition to being dependent upon the temporal relations of the distractor, is also dependent upon the conscious attitude of the *S* during the distraction, a passive attitude being conducive to less or no distraction, while the active attitude produces distraction. During the practice series the *S*s became thoroughly habituated to the motor and counter, finally reporting that these factors were not disturbing in the least. This would seem to indicate a passive attitude on the part of the *S*s; and if the above hypothesis is correct these noises caused very little or no variation from the performance which the *S*s would have carried on in a sound proof room and with a "noiseless" apparatus. The writer is well aware of the ability of the individual to keep up his efficiency of performance as measured by output, and even increase it, though intense distracting stimuli are applied (20). What this costs from the physiological standpoint is still a matter of research. So the question of the noise constants in our experiment will probably not be settled until some study of metabolic cost is made comparing the procedure we employed with one free from such sounds.

[25] Thus 12 syllables were actually learned. Some investigators in memory experiments appear to have used 12 syllables but have allowed the first syllable to act as a cue for the second, and thus have really learned only 11 syllables. In comparing studies this difference in technique needs to be considered.

[26] The rate of presentation prevented verbal reviews during learning in the early part of the experiment. As the *S*s became more adept it might have been possible for the reviews to occur, but the *S*s refrained from attempting it.

of the 12 syllables.[27] In recording the number of repetitions to attain the criterion, the customary procedure of counting the number of repetitions up to, but not including the correct anticipation was employed.

The *determination of the attainment of the criterion* of learning was left solely to the *S*. When the *S* had made a correct anticipation of the list he pressed his key as the asterisks appeared again.

Accent. As a result of the practice experiments no accent was employed in the main experiment itself. In the practice periods the *S*s found no tendency to accent entering. Subject *V* tried various accents and found them distracting. This seems rather unusual that accent did not force itself into the learning, especially when other investigators have reported it repeatedly and have had to control it. It may be that the intermittent movement of the drum tended to give all the syllables the same emphasis and thus made it possible for the *S*s to learn without accent. Whether the particular rate of presentation has anything to do with the tendency to accent is a question.

Rate of presentation. The rate of presentation was 1½ seconds per syllable. In practice experiments this was determined as the optimum rate for these *S*s. Several factors are to be considered in selecting the rate of presentation in the learning of nonsense syllables. (1) The time should be long enough to allow full apprehension, and in our case, audible reading of the syllable. (2) It should be short enough that meaning does not readily arise.—Several studies have used intervals of 2 seconds which would seem to be too long for *S*s of college caliber. (Note criticism of Glaze, Note 8, p. 8.)—(3) It should be short enough that attention cannot shift to any great extent and efficiency still be maintained, thus

[27] See E. B. van Ormer and K. M. Dallenbach, A Frequent Error Concerning Ebbinghaus' Experiments on Obliviscence, American Journal of Psychology, 43, 1931, 706–707. In spite of statements to the contrary, it is a matter of fact that Ebbinghaus used two correct anticipations in succession as his criterion in his main experiments on retention as a function of time, just as did Radosavljevich. This makes fallacious any attempt to explain the disparity between these two investigations as due to difference in criterion of learning.

Since the criterion of one correct anticipation has become the more customary procedure in nonsense syllable memorizing and since it was felt that it might be more of an incentive to the *S* than a requirement of two in succession, the former criterion was adopted. The writer is of the opinion that the two criteria would give approximately the same results if employed with well practiced *S*s. Ebbinghaus (7, p. 109) himself reports results which offer a comparison of the two criteria for 24 hours and practically the same percents of retention are obtained for both in his case.

forcing the *S* to concentrate fully upon the task in hand in order to maintain efficiency. (4) It should be short enough to prevent verbal reviews between syllables. The time intervals of 2 seconds or longer for college students are to be criticized again on this score. Probably some of the confusion over the question of primacy and recency as factors in memorizing serial items is due to failure to observe this point.

The interval between presentations of a given list was determined by the speed of presentation employed. There were 13 spaces on the drum—the 13th for the asterisks, see p. 16.—So the interval between presentations would be the time required to present one extra space, or in this case 1½ seconds.

An experimental period consisted in the learning, or relearning as the case might be, of three 12-syllable lists with a 45 second pause between the learning of successive lists.[28] This pause was timed by a manually operated 1/5 second stop-watch and was as short a time as the manipulation of the apparatus made feasible. During the pause it was necessary to: record the readings of the counter and the clock; to reset both; to take off the drum containing the list just learned; and to put on the drum containing the next list. A little care was required in adjusting the drum so that the electrical contacts worked properly. The *S* was instructed not to think of the syllables during this pause, and to employ his consciousness with as few thoughts as possible. This the *S*s seemed rather able to do, relaxing generally and often resting the eyes by closing them. Practically no trouble was experienced with the syllables perseverating during the pause. It is desirable, of course, to make this pause as short as possible in order to prevent perseveration and voluntary reviews.

The value of learning more than one list at a sitting and in quick succession is twofold: (1) when more than one list is learned, perseveration during the retention periods is less likely to occur, probably due to some sort of retroactive inhibition, proactive inhibition, and general confusion or interference of the lists. The *S*s in this study had practically no trouble with perseveration; and

[28] It had at first been thought to learn 5 lists at one sitting; but it became apparent that the learning of that number would employ possibly 25 to 30 minutes, which would result in very little time being left for a 1-hr. period of sleep, or else necessitate the relearning of the first list at about an hour and 25 minutes after learning, rather than an hour and 10 or 12 minutes, depending upon the method of computing the retention interval. It is doubtful whether the patience and perseverance of the *S*s could have been prevailed upon to the extent of relearning 5 lists at the night periods of relearning.

even found during the practice series that voluntary review was practically impossible. It had been thought that a measure of the number of syllables correctly anticipated on the first presentation of relearning might be obtained and compared with the relearning score. However, in the practice series it was found that most of the time *S* could not anticipate a single syllable on the first presentation of relearning. However, it is doubtful if perseveration can be eliminated when only one list is used. This is a severe criticism of all studies in which only one list was learned at a sitting. (2) The learning of more than one list at a sitting gives a more reliable measure of the *S's* learning performance at that time, both the variation due to inequality of lists and to momentary variations in the *S's* organic condition, attention, and motivation, being somewhat compensated for, if the assumption is correct that they occur as often in the plus as in the minus direction.

Number of experimental periods. In determining this question two considerations are involved. (1) It is desirable to have a sufficient number of experimental periods to give a statistically reliable measure of retention for each time interval for each *S*. (2) In this study it is desirable to not have so many periods that the patience and perseverance of the *S* is disturbed and point 1 partially vitiated thereby. Since Jenkins and Dallenbach got reliable results using the method of retained members, with 8 experiments per retention interval and learning only 1 list at a sitting, it seemed reasonable to suppose that in our case, learning 3 lists at a sitting, 8 experimental periods for each time interval of both sleep and waking for each *S* should give fairly satisfactory results and still not be too severe a strain on the *S*s. Accordingly this was the number of experimental periods employed in the testing of retention. Additional experiments were conducted to test diurnal variation in learning performance, see p. 21.

Times of day at which experiments occurred. Since the night (sleep) learnings had to come to a conclusion at a time when noise in adjacent apartments had subsided, these times more or less determined when the day (waking) learnings could begin if the *S*s were to get approximately the same amount of sleep each night. Night learning occurred between the times of 11:00 P. M. and 12:30 A. M. The day learnings occurred between 9:00 A. M. and 10:15 A. M. That is, the learnings were always completed by 12:30 A. M. and 10:15 A. M. The relearnings occurred at the designated time intervals following the learning, the times of

relearning for the sleep periods being on the average about 12:30 A. M., 1:30 A. M., 3:30 A. M., and 7:30 A. M.; and for the waking periods: 10:30 A. M., 11:30 A. M., 1:30 P. M., and 5:30 P. M.

Computation of the retention intervals. The time of the relearning was computed from the end of the learning of the third list, this seeming to be about as satisfactory a procedure as any. For the timing of the sleep periods an ordinary alarm clock was deemed accurate enough, it being set, at the end of the learning, for the full time period. Since 7 to 10 minutes were employed in the *S*'s waking, see p. 21, each time interval for the sleep periods was to that extent longer than the corresponding waking periods. It seemed advisable not to cut 7 to 10 minutes off the hour period of sleep, it probably often taking the *S* 5 to 10 minutes to get to sleep anyway. Since there would naturally be some variation in the time required for waking the *S*, it seemed best to set the clock for full hours in all cases, and not attempt any correction of the time.

Order of experimental periods. Experiments were conducted daily—with exceptions as noted later. Usually 1 *S* had a waking retention period and that night a sleep retention period. The next day the other *S* had a waking retention period and that night a sleep retention period. This alternation was continued throughout the experiment, except when one or the other of the *S*s might learn for two or three days in succession, due to various irregularities in the other's physical condition. The occurrence of the different time intervals of retention was arranged according to four planned haphazard orders, a different order for sleep and waking for the same *S* and different orders for the two *S*s. This prevented the *S* from figuring out the relearning time before being told as explained below. If there should be a practice effect these orders were so arranged that each of the four time intervals would profit by it to practically the same degree.

S's knowledge of relearning time. At the conclusion of the learning of the last list *S* was told the length of the retention interval. This was true for both sleep and waking experiments.[29]

Practice effect. In order to eliminate practice from the main experiment as much as possible the *S*s learned practice lists con-

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[29] Jenkins and Dallenbach and Dahl told the *S* for the waking experiments but did not tell him for the sleeping ones, being afraid that this knowledge would affect his ability to go to sleep, especially for the short periods. In a few practice tests of relearning at night our *S*s were not troubled by the knowledge of the time of waking. Accordingly it was decided to tell the *S* in all cases, keeping the comparison of sleep and waking as free from other factors as possible.

structed in the same way as the lists used in the main experiments. This learning was kept up until no definite improvement was noticeable in the total number of repetitions required for the learning of three lists at one sitting. This practice required 15 to 17 experimental periods for each *S* and the learning of around 50 lists apiece. The results are reported on page 26. The planned haphazard order of the time intervals in the main experiment also helped to equalize what practice effect occurred in the main experiment.

Diurnal variation. In order to take care of this, *learning* experiments were conducted at the average time of the various time intervals of learning for both sleeping and waking. Eight of these check experiments, each consisting of the learning of 3 lists, were conducted for each *S* for each time interval of waking. On the day that one *S* had a 1 hour retention experiment, the other *S* did a 2 hour check experiment; when the first *S* had a 2 hour retention experiment the other did a 4 hour check test; and when the first *S* had a 4 hour retention experiment the other *S* did an 8 hour check test. The 1 hour checks, of course, required additional mornings. The day checks for both *S*s were run off in this way and scattered at random throughout the course of the experiment—as the above plan permitted—so that the different times received about the same treatment from any practice effect that may have occurred. In the case of the night checks, 4 learning experiments, each consisting of the learning of 3 lists, were conducted for each *S*, at each time interval of sleep. For these experiments the *S* would go to sleep and then be awakened at 1, 2, 4, or 8 hours to learn for the first time a set of lists. These checks required additional nights. It had been supposed that there would be sufficient interruption of the night retention experiments to permit the night checks to be made at random times throughout the experiment. Such was not the case, however, and in anxiety to get the retention experiments themselves completed, the night checks were not scattered throughout the experiment as well as the day checks. Half of the 8 hour night checks occurred in the first half of the experiment and the rest at the end of the experiment; while the other night checks occurred during the last half of the retention experiments. What effect this undesirable procedure may have will be discussed under the results.

The awakening of the S. When the alarm went off the *E* always awakened the *S* until a verbal response was received. The *S* was then allowed to arise voluntarily and dash cold water on his

face, and before starting the experiment, required to testify to being fully awake.—7 to 10 minutes usually elapsed from the time of the alarm until the beginning of the experiment.—With this procedure no results were obtained such as Jenkins and Dallenbach reported in which *S* was awakened, recalled the syllables, and then remembered nothing about it the next day; nor were we troubled with "sleep drunkeness" (Schlaftrunkenheit) as Dahl reported, especially for the 1 and 2 hours. In our case the fact that both *S*s were very strongly motivated toward the experiment may have had something to do with the ease of getting awake.

Bodily and mental health. An effort was made to keep these factors as constant as possible; and when any definite disturbance occurred the experiments were suspended temporarily. When the *S* felt especially the need of additional sleep, he was allowed to sleep in the day time for a short period.—Of course he never slept in the day time until all his experiments for that day time were over.—We tried to abide by the criterion that the *S* should feel reasonably well and rested at any time during the day that he did any learning. As mentioned on p. 19, we tried to see that the *S* got approximately the same amount of sleep each night.

Experimental period disturbed. If for any reason an experimental period became definitely disturbed by environmental factors or by mental distractions within the *S*'s own consciousness, the results of that period were thrown out, that experiment being repeated the next time, or soon after, an attempt being made to deceive the *S* in the matter.—The records of 3 experiments had to be excluded in the case of *G*, and 7 in the case of *V*.

Nature of the interpolated waking activity. The usual activities of a graduate student and his wife living in New York City were interpolated during the waking periods as time and the conditions of the experiment permitted. It is not conceivable that the difference in such activities should make any difference between the results of the two *S*s.

Place of the experiment. The experiment took place in a top floor apartment in the Columbia University district of Manhattan. A special room was set aside for the actual experiment.

Date of the experiment. The practice experiments occurred for the most part during the month of November 1931, a few occurring in the preceding September and October. The main experiments began December 11, 1931, with a three weeks' recess at Christmas, one week being due to sickness, and continued from then on, as

the conditions of the *S*s warranted, until April 14, 1932. The Christmas recess apparently had no effect upon the learning performance, as the data on the practice effect in the main experiment show. (See page 26.)

Improvement on previous memory studies. (1) The use of a more precise and exacting memory method than has been used in the previous studies of Jenkins and Dallenbach, and of Dahl is of significance. The method of savings usually gives a finer scale by which the percentages of retention can be measured than do the other memory methods. It will also frequently indicate some retention when methods of free recall fail to do so. By our method it is also possible to check diurnal variation in learning performance and get a measure of the absolute progress of forgetting for the time intervals studied, making it possible to compare our results with those of Ebbinghaus (7), Radosavljevich (24), Finkenbinder (8), Luh (16), and Boreas (2). (2) Our procedure of learning 3 lists at one sitting has the advantages as mentioned earlier (see p. 18), as well as the additional advantage of making possible the fractionation of the data, all this being an improvement on previous studies of sleep and retention. (3) The *S*s of this investigation would seem to be more thoroughly practiced before beginning the experiment than any previous *S*s used in retention experiments with the possible exception of Ebbinghaus.[30] (4) The lists of syllables constructed by us would seem to be more easy to pronounce and more nearly equivalent in difficulty than any other set of lists previously constructed for such protracted experimentation. Moreover, they seem equally well adapted to either visual or auditory presentation—something which cannot be said of Glaze's (11) association tables—, and more free from meaningful words (see p. 8). (5) To the writer's knowledge no previous investigator of retention has reported a calibration of his presentation apparatus. (6) The use of the sound screen as a valuable precaution in nonsense syllable learning experiments apparently is here recommended and employed for the first time. (7) In general, this experiment would seem to have a better control of all conditions than any previous retention experiment, with the possible exception of Ebbinghaus. The *S*s subordinated all their other activity to that of the investigation.

[30] Ebbinghaus (7, page 45), gives no report of his practice experiments, although he says that long preliminary experiments preceded the definite tests of retention, so that the time of increasing skill may be considered as past.

RESULTS

Learning 1st, 2nd, and 3rd lists. Before the diurnal variation in learning performance can be studied it is necessary to ascertain if there is a difference in the number of repetitions required for learning the 1st, 2nd, and 3rd lists of an experimental sitting. If there is no difference each list can be treated as an independent measure in determining the diurnal variation; if there is a difference each experimental sitting must be treated in toto as one measure, using in the calculations the total number of repetitions required for the learning of the 3 lists. Table I gives the average

TABLE I
AVERAGE NUMBER OF REPETITIONS FOR ORIGINAL LEARNING OF 1ST, 2ND, AND 3RD LISTS AT THE VARIOUS LEARNING PERIODS

Time	*Subject G*							*Reliability of greatest Diff.*
	No. Cases	*1st List Aver.*	*MV*	*2nd List Aver.*	*MV*	*3rd List Aver.*	*MV*	*($D/PE_{diff.}$)*
9:30 A. M. ...	32	9.3	2.6	10.2	2.1	9.9	2.0	1.80
10:30 A. M. ...	8	10.5	3.6	9.8	2.5	9.9	2.4	0.53
11:30 A. M. ...	8	9.8	1.8	9.9	2.4	11.0	1.0	1.94
1:30 P. M. ...	8	9.9	1.4	9.5	2.8	11.3	2.5	1.59
5:30 P. M. ...	8	11.4	2.0	8.5	2.3	10.4	2.7	3.19
11:30 P. M. ...	32	10.5	2.4	10.9	1.9	11.6	2.0	2.34
12:30 A. M. ...	4	9.0	1.5	10.5	1.3	9.3	2.3	1.79
1:30 A. M. ...	4	7.8	1.3	9.8	1.8	11.3	1.3	4.49
3:30 A. M. ...	4	10.0	3.0	10.8	1.9	8.3	0.8	2.91
7:30 A. M. ...	4	7.8	2.8	9.3	1.8	11.0	2.5	2.01
				Subject V				
9:30 A. M. ...	32	11.2	2.8	11.6	2.1	10.8	2.7	1.57
10:30 A. M. ...	8	14.9	5.2	12.4	1.6	11.4	1.9	2.12
11:30 A. M. ...	8	13.4	3.1	12.5	4.3	11.4	2.0	1.80
1:30 P. M. ...	8	13.5	3.6	13.1	2.4	11.3	2.3	1.72
5:30 P. M. ...	8	11.5	2.9	11.8	2.5	11.8	2.0	0.26
11:30 P. M. ...	32	13.3	2.9	14.1	3.2	12.8	2.2	2.24
12:30 A. M. ...	4	14.8	5.2	13.8	1.9	12.8	3.8	0.73
1:30 A. M. ...	4	16.3	3.9	10.3	1.3	13.5	2.0	3.45
3:30 A. M. ...	4	13.0	5.5	12.8	2.2	10.5	0.8	1.07
7:30 A. M. ...	4	10.8	1.8	10.5	0.5	12.8	1.8	2.91

number of repetitions required for learning the 1st, 2nd, and 3rd lists and the reliability of the greatest difference among these three values[31] for each *S* for each learning period. For *V* only one difference approaches 4 times its probable error, viz. the difference between the 1st and 2nd lists at 1:30 A. M., the D/PE being 3.45.

[31] Using the formula $PE_{aver.}$ equals $.8453\ MV/\sqrt{N}$, the $PE_{aver.}$ has been calculated and from it the $D/PE_{diff.}$

In view of the fact that this value is based on the learning of only 4 lists, which are so few in number that it is doubtful whether the reliability formula is applicable,[32] and in view of the fact that the differences at all of the other periods are not reliable, it is thought that this single case is without significance. In the case of *G* the only differences that are greater than 3 times their PE are those at the 5: 30 P. M. and 1: 30 A. M. learning periods. Since the determinations for these two periods are based on 4 and 8 cases respectively, the differences again do not appear significant, particularly as the experiments at 9: 30 A. M. and 11: 30 P. M., which afforded opportunity for reliable differences to show themselves, do not give reliable differences for either *S*. In view of these results we feel justified in concluding that a difference in the number of repetitions required for the learning of the 1st, 2nd, and 3rd lists of a sitting has not been demonstrated for our two *S*s. Accordingly, in the calculation of diurnal variation in learning performance each list will be treated as a separate and independent measure.

Practice effect. It had been hoped that the preliminary series of practice experiments[33]—15 sittings or 45 lists for *G* and 17 sitting or 51 lists for *V*—would eliminate practice, but as Fig. 1 shows the practice effect persisted in the main experiment for a considerable time. Every point of the broken curves represents the total number of repetitions required to learn 3 lists at one sitting. The solid curves are based on the average number of repetitions for 3 experimental sittings, or the average number of repetitions to learn 3 sets of 3 lists. The first of the vertical lines (line A) cutting the curves represents 1 month's intermission in the practice series; the second line (line B) represents two week's intermission between the conclusion of the practice series and the beginning of the main experiment; the third line (line C) represents a 3 week's intermission in the main experiment, two weeks consisting of the Christmas holidays and the third of sickness. Aside from the evidence that practice entered the main experiment, these curves are of value in showing the great amount of improvement which can take place from continued practice in a particular function. Dur-

[32] Regarding this point see Garrett, H. E., Statistics in Psychology and Education, 1926, p. 142.

[33] Some of these practice experiments took place in the morning and some at 11: 30 P. M. All the points of the preliminary practice curves of both *S*s were corrected to 9: 30 A. M. as a base, using the coefficients or weights of diurnal variation mentioned later.

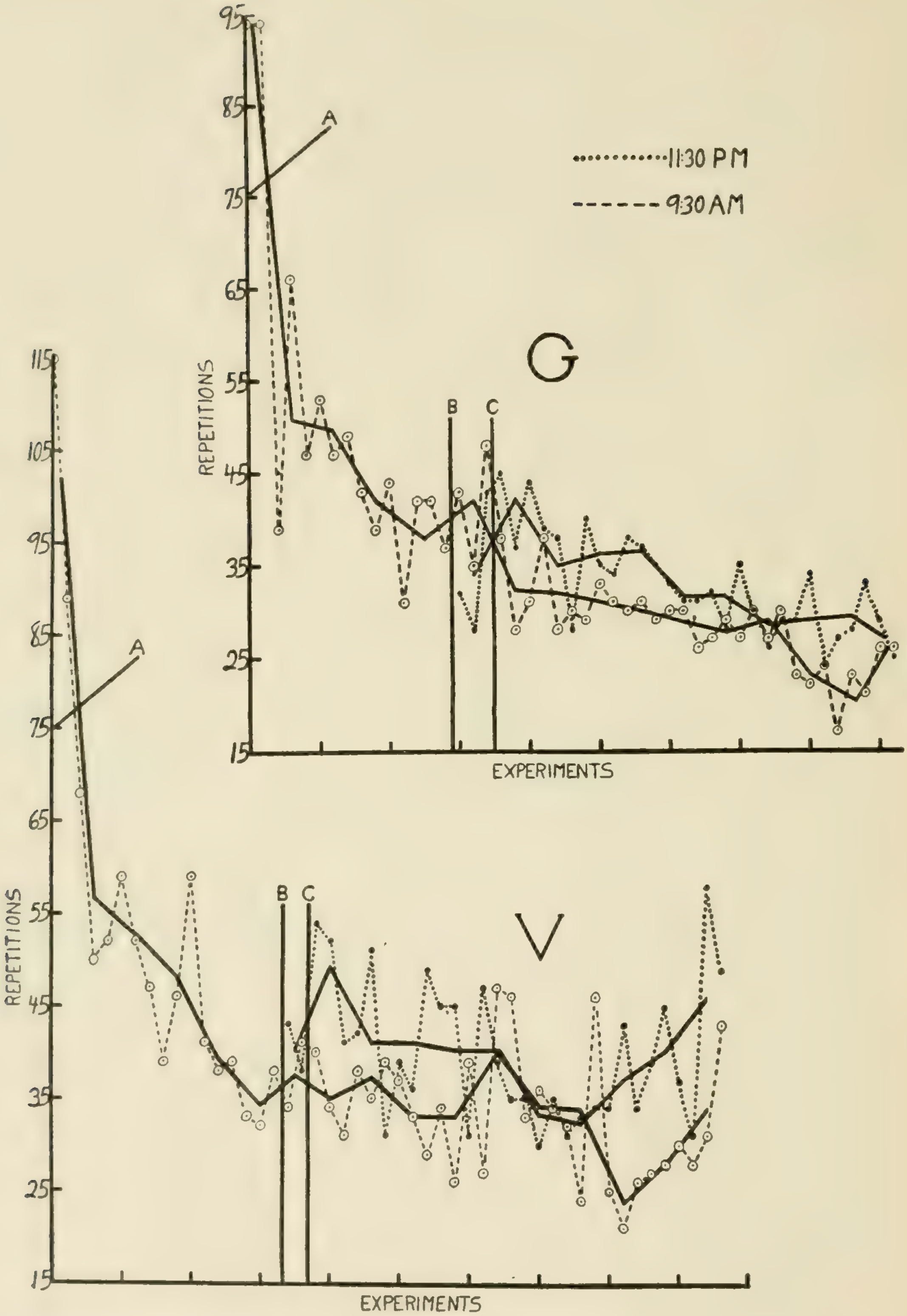

Fig. 1. Practice curves.

ing the practice series the *S*s were definitely trying to improve and were shown their results. During the main experiment the *S*s were simply trying to do their best, with no motivation to improve and no direct knowledge of their own results, the *S*s of course, being unable to keep from estimating in a general way whether they had done well or poorly. Comparison of introspective reports and objective data indicates that the *S*s were frequently in error in their subjective estimates of whether a given list had taken longer than another one of the same sitting. Of course each *S*, while acting as *E* noticed (although the records for each experiment were kept separate and not looked at until the conclusion of the whole investigation) that the other *S* was gradually improving. Introspective reports show that *V* had a vague suspicion that he was learning the lists in fewer repetitions as the experiment progressed, not being conscious of any particular time at which the improvement started taking place. *G* reported that the lists seemed gradually easier to learn, although she was not of the opinion that she was learning them in fewer repetitions. In fact, because she found the syllables becoming gradually easier to pronounce and becoming easier to recognize on the repeated repetitions, *G* was frequently deluded into the belief that it was taking her longer to reach the criterion of mastery. That is, in the earlier part of the experiment it took *G* 2 or 3 repetitions, as she reports introspectively, to get the pronunciations of the syllables, then possibly 2 or 3 more until she was able to recognize fully the syllables as belonging in the position in which they appeared. Apparently as she improved in performance these first two processes were accomplished in fewer repetitions and the time from them to mastery was still the same or greater, giving the illusion of the learning taking longer.

As nearly as the *S*s can tell introspectively the improvement which occurred in the practice experiments consisted in the following factors at least: (1) an improvement in ability to pronounce the syllables quickly and accurately; (2) general adjustment to the situation of the experiment, noise of motor, fixation of aperture as syllables passed by, etc.; (3) elimination of an excessive emotional reaction to the situation; (4) less shifting of attention from the actual learning at hand; (5) a lessening of the muscular tension, or the maintaining of a more relaxed bodily position.[34] *G* reported

[34] Both *S*s noticed and reported that they had to be careful not to relax too much or attention shifted from the learning. It is significant that *G*, who did not know of the recent work on tension, should have made this observation.

in addition, what was described as an improvement in ability to recognize the syllable as familiar and as the one belonging in that position. *G* noticed the various factors of improvement continuing throughout the experiment proper; *V*, on the other hand cannot explain any improvement which occurred in his case during the main experiment. As indicated above, he had only a suspicion that he was improving. Possibly the greater fluctuation of his performance helped to conceal from him more definite indication of his improvement.

What influence does this practice effect have upon comparing the various experiments for sleep and waking and for the various time

TABLE II

Average Number of Repetitions Per List Required for Original Learning at the Various Time Intervals. (Each List Treated as a Separate Measure.)

Time	*Subject G* Aver.	*MV*	*No. Cases*	*Subject V* Aver.	*MV*	*No. Cases*
9: 30 A. M.	9.8	2.3	96	11.2	2.5	96
10: 30 A. M.	10.0	2.8	24	12.9	3.2	24
11: 30 A. M.	10.2	1.8	24	12.4	3.2	24
1: 30 P. M.	10.2	2.2	24	12.6	2.8	24
5: 30 P. M.	10.1	2.7	24	11.7	2.5	24
11: 30 P. M.	11.0	2.1	96	13.4	2.7	96
7: 30 A. M.	9.3	2.6	12	11.3	1.6	12
	1-, 2-, and 4-hour sleep checks compared with 2nd half of 9: 30 A. M. experiments.					
9: 30 A. M. (2nd half)	8.5	1.9	48	10.6	2.5	48
12: 30 A. M.	9.6	1.8	12	13.8	3.6	12
1: 30 A. M.	9.6	1.9	12	13.3	3.1	12
3: 30 A. M.	9.7	2.2	12	12.1	3.0	12

intervals? Tables V and VI give the average number of repetitions required for learning the lists used in measuring the retention for the various time intervals of the experiment. All 11: 30 P. M. learnings have been corrected to be comparable to the 9: 30 A. M. learnings, using the diurnal variation as discussed in a following section. Considering in Tables V and VI the averages of the total number of repetitions required for the original learning of the 3 lists combined, it is clear that for both *S*s there is no significant difference between the depth of impression (repetitions required for original learning) of corresponding periods of sleep and of waking, which difference can be attributed to practice effect. A further study of the tables and calculation of reliabilities shows

that there is, moreover, no reliable difference between the number of repetitions required for learning at any two points of the same curve or different curves. These facts would seem to indicate that the planned haphazard order of conducting the experiments dis-

TABLE III

DIFFERENCES AND $D/PE_{diff.'s}$ FOR THE AVERAGES OF TABLE II. THE UPPER RIGHT HAND TRIANGULAR BLOCK GIVES THE DATA FOR *G*; THE LOWER LEFT HAND TRIANGULAR BLOCK GIVES THE DATA FOR *V*

	9:30 A.M.	*10:30 A.M.*	*11:30 A.M.*	*1:30 P.M.*	*5:30 P.M.*	*11:30 P.M.*	*7:30 A.M.*
A.M.							
9:30 D		0.2	0.4	0.4	0.3	1.2	0.5
D/PE		0.39	1.08	0.93	0.59	4.44	0.76
10:30 D	1.7		0.2	0.2	0.1	1.0	0.7
D/PE	2.88		0.35	0.33	0.15	1.96	0.89
11:30 D	1.2	0.5		0.0	0.1	0.8	0.9
D/PE	2.03	0.64		....	0.18	2.22	1.29
P.M.							
1:30 D	1.4	0.3	0.2		0.1	0.8	0.9
D/PE	2.64	0.41	0.27		0.17	1.91	1.22
5:30 D	0.5	1.2	0.7	0.9		0.9	0.8
D/PE	1.04	1.71	1.00	1.41		1.80	1.01
11:30 D	2.2	0.5	1.0	0.8	1.7		1.7
D/PE	6.88	0.83	1.67	1.51	3.47		2.58
A.M.							
7:30 D	0.1	1.6	1.1	1.3	0.4	2.1	
D/PE	0.22	2.39	1.64	2.10	0.69	4.67	

1-, 2-, and 4-hour sleep checks compared with 2nd half of 9:30 A. M.

	2nd half 9:30 A.M.	*12:30 A.M.*	*1:30 A.M.*	*3:30 A.M.*
9:30 A.M. D		1.1	1.1	1.2
(2nd half) D/PE		2.20	2.16	2.03
12:30 A.M. D	3.2		0.0	0.1
D/PE	3.44		....	0.14
1:30 A.M. D	2.7	0.5		0.1
D/PE	3.29	0.43		0.14
3:30 A.M. D	1.5	1.7	1.2	
D/PE	1.90	1.49	1.14	

tributed the practice effect rather well throughout the various groups of the experiments.

These practice curves are also of value, as will be pointed out later, in showing the definite superiority of the 9 : 30 A. M. learning over the 11 : 30 P. M. learning.

From the standpoint of differences between the two *S*s the curves are salient proof of the greater consistency of performance on the part of *G*. Whether the much more pronounced fluctuations in *V*'s curves are due to anxiety over the investigation or to a distinct difference in temperament is only a matter of conjecture. It would seem plausible that an investigator's knowledge of the nature of the experiment, of the results of previous investigators, and his anticipation of the results might be sufficiently disturbing, as Ebbinghaus pointed out years ago, to cause more than usual fluctuation in his performance.

It should be noted that these practice curves are not a complete picture of the practice which the *S*s received. Interpolated at various places between the experiments indicated on these curves occurred the experiments conducted for checking diurnal variation of learning performance. These check experiments are not included in the curves because a comparison of 9 : 30 A. M. and 11 : 30 P. M. learnings was desired. Including these check experiments and experiments which had to be thrown out because of disturbing factors, each *S* participated in over 162 experimental sittings, involving the learning of over 486 different lists of 12 syllables each.

Diurnal Variation. Since, as mentioned before, no reliable difference was found between the number of repetitions required for learning the 1st, 2nd, and 3rd lists of one sitting, each list was treated as a separate and independent measure in determining the diurnal variation in learning performance. Table II gives the average number of repetitions per list required for learning at the various time intervals for each *S*. Since the 1-, 2-, and 4-hr. night checks of learning performance were conducted in the second half of the investigation (see p. 21) and in view of the practice effect which occurred, these night checks are compared with the average of the second half of the 9 : 30 A. M. learnings rather than with the average of all of the 9 : 30 A. M. learnings. Examination of Fig. I would seem to make this a justifiable procedure. Table III compares the differences between the average number of repetitions per list required for learning at the various time intervals. For each comparison is given the difference between the two averages

and also the quotient of $D/PE_{diff.}$. The upper right hand triangular block of the table contains these comparisons for subject *G* and the lower left hand triangular block contains those for subject *V*.

In determining corrections of the number of repetitions required for relearning at the various time intervals and for learning at 11 : 30 P. M., the averages for all times were compared with the average for 9 : 30 A. M. as a base and if necessary corrected to be comparable with it. For both *S*s there is a statistically reliable difference between the learning performance at 9 : 30 A. M. and at 11 : 30 P. M. For *G* the performance at 11 : 30 P. M. is approximately 11% of itself lower (it taking longer to learn) than that at 9 : 30 A. M. Accordingly all the learnings for *G* at 11 : 30 P. M. have been reduced by 11% before calculating the percentage of forgetting. For *G* the day checks of learning performance at 10 : 30 A. M., 11 : 30 A. M., 1 : 30 P. M., 5 : 30 P. M., and the night check for 7 : 30 A. M. do not differ significantly from 9 : 30 A. M. and accordingly the relearnings at those times are left uncorrected. When the night checks for *G* at 12 : 30 A. M., 1 : 30 A. M., and 3 : 30 A. M. are compared with 9 : 30 A. M. partially reliable differences are found, the $D/PE_{diff.}$ being slightly over 2.0 in all three cases. Since these differences are all in the same direction and of about the same value, it seems better to recognize them than to ignore them. So the relearnings for these times have been reduced by 12% of themselves.

In the case of *V* the reliable difference between the learning performance at 9 : 30 A. M. and 11 : 30 P. M. necessitates a reduction of the learnings at the latter time by 16% of themselves. When the day check experiments at 10 : 30 A. M., 11 : 30 A. M., and 1 : 30 P. M. are compared with 9 : 30 A. M., the reliabilities of their differences ($D/PE_{diff.}$) are 2.88, 2.03, and 2.64 respectively. Since these differences are all in the same direction and based on 24 cases, they too, should probably be accepted rather than ignored. Accordingly the relearnings at 10 : 30 A. M. are reduced by 13% of themselves, those at 11 : 30 A. M. by 10%, and those at 1 : 30 P. M. by 11%. The differences between these three times of relearning are not significant; but it seems best to take the percentages which the results give and not to correct all three by, say 11%. The 5 : 30 P. M. learnings and the 7 : 30 A. M. learnings are not significantly different from 9 : 30 A. M. The comparison of the night checks at 12 : 30 A. M., 1 : 30 A. M., and 3 : 30 A. M. with the second half of

the 9:30 A. M. learnings gives differences whose reliabilities ($D/PE_{diff.}$) are 3.44, 3.29, and 1.90 respectively. Although this is not complete reliability, the differences are comparatively large and in the same direction, and if the corrections at 10:30 A. M., 11:30 A. M., and 1:30 P. M. are accepted, the corrections for these night hours must also be included in the calculations. So the relearnings at 12:30 A. M. are reduced by 23% of themselves, those at 1:30 A. M. by 20%, and those at 3:30 A. M. by 12%. Table IV makes possible a comparison of the diurnal variation in learning performance for the various times of learning and relearning for each *S*.

TABLE IV

PERCENTAGES BY WHICH THE LEARNING PERFORMANCE AT THE VARIOUS TIMES OF LEARNING ARE OF THEMSELVES BELOW THE LEARNING PERFORMANCE AT 9:30 A. M. (CALCULATIONS BASED ON AVERAGE NUMBER OF REPETITIONS REQUIRED FOR ORIGINAL LEARNING.)

Times	*Percentages for G*	*Percentages for V*	*No. Cases (for each S)*
9:30 A. M.	00.0	00.0	96
10:30 A. M.	00.0	13.0	24
11:30 A. M.	00.0	10.0	24
1:30 P. M.	00.0	11.0	24
5:30 P. M.	00.0	00.0	24
11:30 P. M.	11.0	16.0	96
12:30 A. M.	12.0	23.0	12
1:30 A. M.	12.0	20.0	12
3:30 A. M.	12.0	12.0	12
7:30 A. M.	00.0	00.0	12

The percentages in the table indicate how much the learning performance at a certain time is of itself below the learning performance at 9:30 A. M. The striking things about this table are the failure of any variation during the daytime in the case of *G* and the failure of 5:30 P. M. to show any variation from 9:30 A. M. in the case of both *S*s. This latter result fails to confirm the result of Ebbinghaus, who found his performance at 6–8 P. M. to be 12% of itself below that at 10–11 A. M.; and is against the argument put forth by Radosavljevich (24), Bean (1), Finkenbinder (8), and Boreas (2) that fatigue would cause relearnings at 5:30 P. M. to take longer and consequently cause a drop in retention curves at the 8-hr. interval if those relearnings took place at 5:30 P. M. without corrections for diurnal variation.

Calculation of percentages forgotten. In all records of the number of repetitions required for learning or relearning the usual

Insert p. 33 after line 5.

ting for the 1st, 2nd, and 3rd lists for each time interval of sleep and of waking for each S, and the average percentages of forget-

method of recording one less than the number of repetitions at which the first correct anticipation of the list occurred has been followed. All percentages of forgetting are the ratios of the number of repetitions required for relearning to the number required for learning. Tables V and VI give the average percentages of forgetting based on the 3 lists combined, the calculation, in the latter case, being the total number of repetitions to relearn the 3 lists divided by the total number of repetitions to learn the 3 lists; tables V and VI also contain for each *S* the average number of repetitions, at the various time intervals of sleep and of waking, required for learning the 1st, 2nd, and 3rd lists, and the 3 lists combined. The 11:30 P. M. learnings given in tables V and VI have been reduced by the proper percentages to make them comparable to the learning at 9:30 A. M. Before any percentages of forgetting were calculated certain of the relearnings were also corrected as indicated in the preceding section.

Retention of the 1st, 2nd, and 3rd lists. In considering tables V and VI one is at once struck with the irregularities of the percentages for the separate lists. An examination of the MV in these tables with a calculation or estimation of the $D/PE_{diff.}$ will show that the irregularities or deviations of the individual curves from the combined curves are not, with one exception, statistically reliable, and are probably due to various disturbing factors which exert their influence in view of the small number of measures from which the individual curves are obtained. The one irregularity which seems more reliable is the junction of the sleep and waking curves for *G* in the 3rd list at the 8-hr. interval. Here the MV's are approximately the same size and in comparison with the rest of the data, relatively small. The junction of the sleep and waking curves for *V* in the 2nd list at the 8-hr. interval does not show the same consistency, the MV for sleep being twice that for waking. For both *S*s the 8-hr. period in the 1st list is reliably favored by sleep. For *V* the 4-hr. period in the 2nd list is definitely in favor of sleep. The other differences which appear to favor sleep are of doubtful reliability, being from 2 to 3 times their PE's. Of course in consideration of all these differences one must not lose sight of the limitations of the reliability formula when *N* is only 8. An examination of the average number of repetitions required for learning the various lists used in measuring the different time intervals, shows that these irregularities cannot be explained as due to differ-

TABLE V—SUBJECT G

AVERAGE PERCENTAGES OF FORGETTING FOR 1ST, 2ND, AND 3RD LISTS, AND FOR THE 3 LISTS COMBINED; AND AVERAGE NUMBER OF REPETITIONS REQUIRED FOR ORIGINAL LEARNING, 11:30 P. M. LEARNINGS CORRECTED FOR DIURNAL VARIATION

Time Interval		*1st List*		*2nd List*		*3rd List*		*3 Lists Combined*	
		Sleep	*Waking*	*Sleep*	*Waking*	*Sleep*	*Waking*	*Sleep*	*Waking*
1 hour	Aver. %	55.7	64.5	58.6	45.4	54.4	58.4	56.0	54.2
	MV	14.9	33.2	14.9	9.8	10.7	11.2	9.8	10.8
	Aver. R	8.6	9.0	10.8	10.4	9.6	8.5	29.0	27.9
	MV	1.9	2.5	1.7	1.9	1.8	2.0	4.6	4.4
2 hours	Aver. %	70.8	57.4	56.2	55.8	42.6	69.4	54.2	59.0
	MV	16.0	13.5	11.0	11.1	12.7	26.6	4.1	9.5
	Aver. R	8.8	9.6	10.5	10.4	11.0	9.5	30.3	29.5
	MV	2.3	2.9	1.1	2.0	1.2	2.1	4.0	5.5
4 hours	Aver. %	65.3	77.3	56.1	70.9	49.7	51.2	55.5	62.4
	MV	19.0	25.4	10.7	16.2	6.9	14.4	5.9	7.9
	Aver. R	9.9	8.6	9.5	9.8	10.5	10.6	29.8	29.0
	MV	2.4	2.0	1.9	2.0	1.8	1.4	4.5	3.3
8 hours	Aver. %	50.8	86.7	64.1	93.3	61.3	60.8	57.4	75.4
	MV	9.9	26.5	14.2	30.8	7.1	8.9	3.8	6.6
	Aver. R	10.3	10.0	8.1	10.1	10.3	11.0	28.6	31.1
	MV	1.7	2.5	0.8	2.4	2.0	2.0	3.0	3.2

TABLE VI—SUBJECT V

Average Percentages of Forgetting for 1st, 2nd, and 3rd Lists, and for the 3 Lists Combined; and Average Number of Repetitions Required for Original Learning, 11:30 P. M. Learnings Corrected for Diurnal Variation

Time Interval		*1st List*		*2nd List*		*3rd List*		*3 Lists Combined*	
		Sleep	*Waking*	*Sleep*	*Waking*	*Sleep*	*Waking*	*Sleep*	*Waking*
1 hour	Aver. %	61.9	65.0	62.9	53.9	57.8	61.9	57.9	57.6
	MV	17.3	13.2	27.3	11.2	12.6	10.3	11.8	6.3
	Aver. R	11.0	10.1	10.8	12.4	9.8	8.8	31.6	31.3
	MV	2.4	3.1	2.4	2.8	1.4	1.3	4.2	5.3
2 hours	Aver. %	71.4	60.2	56.8	74.8	60.5	65.7	61.9	64.0
	MV	11.3	19.2	15.1	14.4	18.7	19.5	11.6	7.4
	Aver. R	12.4	11.9	13.1	11.3	10.5	11.6	36.0	34.8
	MV	2.7	3.9	3.5	1.8	2.3	3.1	5.8	6.0
4 hours	Aver. %	73.2	71.6	52.3	74.2	61.6	64.5	60.6	69.4
	MV	17.5	5.8	11.0	8.8	17.6	12.7	11.7	7.5
	Aver. R	10.1	11.8	11.8	11.5	11.7	11.9	33.5	35.1
	MV	2.7	3.2	3.0	2.3	1.6	2.1	6.6	5.4
8 hours	Aver. %	48.4	82.7	69.5	68.2	66.8	86.8	61.0	76.9
	MV	12.8	14.0	14.1	7.4	10.6	23.9	10.6	8.4
	Aver. R	11.2	11.0	11.6	11.1	11.1	11.0	33.9	33.1
	MV	2.0	1.3	1.4	1.2	1.7	3.0	3.7	4.1

ences in depth of impression (number of repetitions required for learning).

Comparison of the retention of the 3 lists combined. In view of the fact that the data available do not make it possible to determine just what effect the learning of the 3 lists in succession has upon the subsequent retention of each of those lists as an individual unit, it would seem best in combining our data for a comparison of sleep and waking, not to consider the retention of each separate list as a single and independent determination. Accordingly our combined curves are computed on the basis of each experimental sitting, the percentage of forgetting recorded in Tables V and VI for the 3 lists combined being, as mentioned before, the quotient obtained from the total number of repetitions to relearn the 3 lists divided by the total number of repetitions to learn the 3 lists. Thus these averages for the 3 lists combined are based on only 8 determinations; but it is well to emphasize that these are 8 much more reliable determinations than those for the curves of the 1st, 2nd, and 3rd lists separately. By taking the total number of repetitions for the 3 lists learned at one sitting, various variables due to the inequality of the lists and to fluctuation in the *S*'s performance during a given sitting, are partially compensated for even though the reliability formula as such is not changed. This fact is demonstrated in the generally smaller MV's which the average percentages for the 3 lists combined have as compared with those for the lists separately.

Fig. 2 gives the reader a picture of the retention curves for each *S* based on the percentages for the 3 lists combined. Upon looking at this figure one is at once struck with at least four facts. (1) The decided advantage of sleep at 8 hrs. (2) The failure of sleep to be superior at 1 hr. (3) The sleep curves are practically horizontal lines from 1 hr. on. (4) The pronounced similarity of the results for the two *S*s. A study of the data in tables V and VI for the 3 lists combined gives a conception of the reliability of the differences between the sleep and waking curves. For *G* the calculations of the $D/PE_{diff.}$ for the comparisons of sleep and waking are as follows: 8 hrs. 7.90; 4 hrs. 2.34; 2 hrs. 1.55; for *V* they are: 8 hrs. 3.94; 4 hrs. 2.12; 2 hrs. 0.51. For both *S*s the fluctuations in the sleep curve after 1 hr. are not statistically significant.

Table VII gives another way of comparing the reliability of the differences between sleep and waking, this time in terms of overlapping. This table gives the percentage forgotten for each *S*, for each experimental sitting—total repetitions to learn 3 lists—at the

various time intervals. The average of each column is then given and the minus signs in front of certain percentages indicate those which are less than the average of the series with which they are compared. This comparison again indicates the superiority of sleep

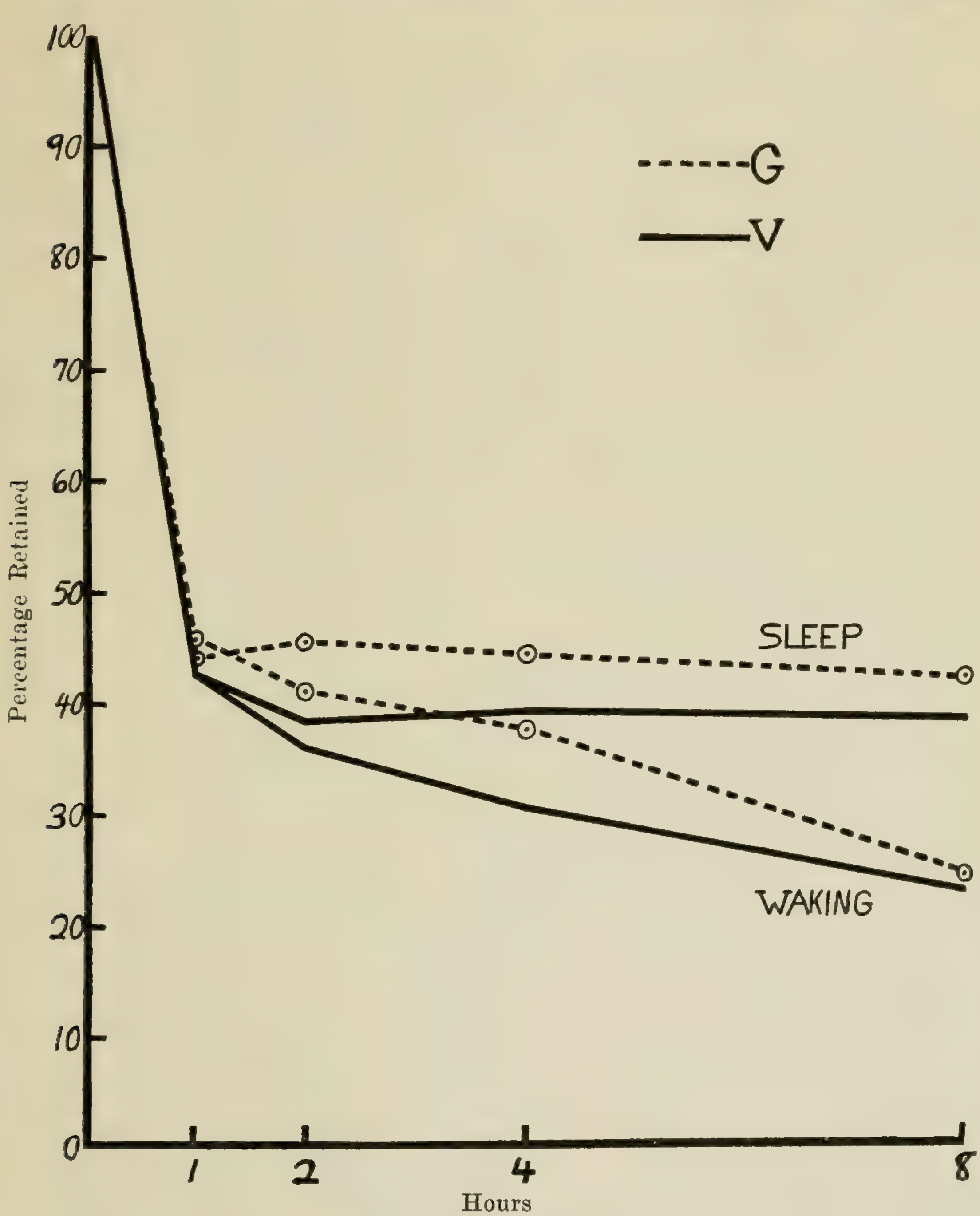

FIG. 2. Retention curves.

at 4 and 8 hours and its slight advantage at 2 hours. The percentages within each column of this table occur in chronological order; so that the reader may compare the first experiment for 1 hour of sleep with the first experiment for any of the other series; and in

TABLE VII

COMPARISON OF PERCENTAGE FORGOTTEN FOR EACH SUBJECT, FOR EACH EXPERIMENTAL SITTING (TOTAL REPETITIONS TO LEARN 3 LISTS) AT THE VARIOUS TIME INTERVALS

	1 hour		2 hours		4 hours		8 hours	
	Sleep	*Waking*	*Sleep*	*Waking*	*Sleep*	*Waking*	*Sleep*	*Waking*
	81.1	–46.5	–48.4	–47.9	–55.1	–47.4	–56.1	85.7
	58.2	–50.0	59.0	–48.4	–38.3	71.4	–65.1	65.8
	–49.4	62.1	–53.5	61.3	–60.2	60.0	–56.1	87.9
Subject G	62.2	–41.4	61.4	63.3	–56.8	56.7	–47.3	77.4
	–42.3	73.1	–51.1	–51.9	64.9	60.0	–61.6	72.4
	–37.6	–44.4	–54.7	68.2	–49.4	73.3	–56.2	70.0
	61.7	–45.8	–58.1	81.0	62.5	73.9	–60.6	66.7
	55.2	70.6	–47.7	–50.0	–56.8	56.5	–56.2	76.9
Aver.	56.0	54.2	54.2	59.0	55.5	62.4	57.4	75.4
Sleep Exps.<Waking Aver.	3		6		6		8	
Waking " <Sleep "	5		4		1		0	
	75.0	64.2	75.2	70.2	–49.1	–60.0	83.1	79.4
	63.2	63.5	67.4	66.2	–54.5	81.9	–68.0	83.9
	81.3	64.4	–48.6	62.3	84.6	80.9	–66.2	85.7
Subject V	–53.5	–53.8	–54.7	77.6	70.0	68.5	–48.5	73.5
	59.1	–49.5	81.6	–55.4	–67.7	64.1	–68.8	–56.5
	–44.7	63.7	69.8	–45.0	–67.1	63.9	–54.4	69.7
	–41.5	–53.2	–55.4	71.3	–52.4	75.4	–55.9	92.0
	–44.9	–48.6	–42.7	64.3	–39.7	–60.4	–42.7	74.1
Aver.	57.9	57.6	61.9	64.0	60.6	69.4	61.0	76.9
Sleep Exps.<Waking Aver.	4		4		6		7	
Waking " <Sleep "	4		2		2		1	

the same manner any other experiment can be compared with any of its chronologically corresponding experiments, for instance, the fifth can be compared with all the other fifth experiments.

The two methods of studying the reliability of the differences between the sleep and waking curves agree in giving sleep a definite advantage at 8 hours. Although the differences at 4 hours are only a little over 2 times their *PE*'s it should be remembered from our discussion above that the reliability here is greater than the formula indicates, since *N* in this case is 8 experiments and not merely 8 lists as in previous comparisons. The comparison of the individual experiments at 4 hours adds some weight to the reliability of this difference. Of course the difference at 2 hours is not at all reliable; but in the light of the general direction of the curve and the similarity of the curves for both *S*s it may be considered at least suggestive of a real but slight difference at 2 hours.

Method of learning. *G* reports that her method of learning was largely by the use of visual images. When she could correctly anticipate a syllable she could usually see the syllable before hand. Many times she had a visual image which included the 2 or 3 syllables following the one which she was saying or was about to say, the visual images keeping ahead of the actual anticipation of the syllables. Occasionally syllables were anticipated in the form of auditory images, without any visual image preceding the apprehension of the syllable itself. Some few syllables just seemed to "say themselves" at times. When this occurred it was likely to be the last 2 or 3 syllables.[35] *G* reports that the first time through the list she was satisfied to be able to pronounce the syllables and decide if each syllable was hard or easy. The first syllable was often hard to learn; while the last three were usually easy. A certain impressive syllable would usually be learned first and then those in the first or second positions preceding or following it would be grouped with it, the rest of the list being gradually built onto this nucleus. The meanings which came with some syllables seemed to be a help in the early part of the experiment; in the latter part of the experiment fewer meanings came and when they did come, seemed to be a hindrance. Syllables containing *z, j,* or *g* seemed especially hard for *G* to learn; while any containing a *w* seemed easy—*w*'s having been very hard during the practice series.

[35] This is probably the case of a muscular habit having been formed before the "ideational" response or conscious response of calling up an image of the syllable. *V* noticed the same thing in his learning, perhaps more frequently than *G*.

V's method of learning was largely a matter of visual images, he frequently being able to visualize the list three or possibly more syllables ahead of the one he was saying. At times, however, syllables were recalled by auditory images, *V* being unable to visualize the syllable. These latter syllables, however, were always transformed into visual images before the list was mastered. A number of times *V* found, much to his surprise, that certain syllables simply "said themselves," as *G* also reported. Introspectively this seemed to *V* to be a muscular habit formed before the "ideational" response, or before the conscious response of calling up an image of the syllable. In the practice series *V* formed the habit of trying to fix the first two or more syllables on the first presentation; usually he could anticipate the first syllable, and many times the first two, on the second presentation. He would hold a visual image of the first syllable in the background of his consciousness while apprehending and pronouncing audibly the rest of the list; then on the second presentation he would anticipate the first syllable, get a visual image of the second, if he had not been able to anticipate it, and go on pronouncing the rest of the list. The next time the third syllable would be held in consciousness, and so on down through most of the list. During this procedure the last one or two syllables would usually become impressed without the above technique, and often other syllables in the last part of the list would seem to impress themselves. *V*'s most efficient procedure, however, seemed to consist in building up the list from top to bottom by this procedure of holding visual images of certain syllables. He is not able to decide whether his attention shifted from this image to each syllable he was apprehending or whether the image remained as a background upon which each syllable was apprehended. Sometimes his attention would shift and he would lose the image which he was holding and as a result would not be able to anticipate that syllable on the next presentation. *V* does not believe that this image was accompanied by any subvocal pronunciation of the syllable which was being visualized. In the early part of the experiment too much attention had to be paid to pronunciation of the successive syllables to seem to allow any opportunity for such subvocal activity. As *V* seemed to improve in ability to apprehend the syllables he might have been able to crowd in some subvocal rehearsal of this image which was being visualized; but he made no effort to do so.[36] It

[36] *V*, at least, in the latter part of the experiment believes that he could have crowded in rehearsals of parts of the lists if he had tried, but he refrained

was of interest to V to notice that if some syllable in the middle or latter part of the list became impressed rather early in the learning it would then tend to come up as a response when no response was ready for a syllable earlier in the list.

Comparison of results with those of other investigators. The results of Jenkins and Dallenbach, and of Dahl agree with our own in showing the favorable effect of sleep at 4 and 8 hours. At the 1- and 2-hour intervals the three studies stand in opposition to one another. Dahl attributed, in part at least, the fact that the sleep curve was lower than the waking at 1 and 2 hours to the "schlaftrunkenheit" of the *S*s at those hours, producing an uncritical attitude and the tendency to give more "yes" answers. As a result of Dahl's comment we took especial pains to get the *S*s thoroughly awake (see p. 21) before doing the relearning at night. Our results show no difference between sleep and waking at 1 hour and a very slight and not reliable difference at 2 hours. Jenkins and Dallenbach reported that frequently their *S*s arose, did the recalling and in the morning had no recollection of it.—This never happened in our investigation.—It may be that their *S*s did the recalling in a trance state and accordingly did better than they would if they had been fully awake.[37] It should be noted that the *Ss* tested by Jenkins and Dallenbach, and by Dahl, did not know when they were to be wakened for testing in the sleep experiments, while our *S*s always knew when they were to be wakened. So far as our *S*s could tell subjectively, the fact that they knew when they were to be wakened did not affect their going to sleep and therefore does not seem to be acceptable as even a partial explanation for the discrepancy at 1 hour between our results and those of Jenkins and Dallenbach. Table IV shows that, in the case of V, learning was definitely more difficult at 12:30 A. M. and 1:30 A. M. than at any other time. This fits in with V's subjective report that it required much more effort for him to do the learning or relearning at these times than at the other times. This condition may be simi-

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from so doing. The speed at the beginning was sufficiently fast to prevent this. V is also of the opinion that he might be able, by change of aufgabe, to produce evidence of the potency of either primacy, recency, or intermediacy of serial position, that is making it his task to hold the images at the beginning, end, or middle of the list first.

[37] A hypnotized subject tested once each for 2-, 4-, 8-, and 24-hr. intervals, by Jenkins and Dallenbach, gave 100% recollection at the first three periods and 80% at the last. As Jenkins and Dallenbach state, authorities on hypnosis disagree as to whether memory is intensified or weakened during this state. See for example, J. M. Stalnaker and L. E. Riddle, The effect of hypnosis on long-delayed recall, J. Gener. Psychol., 1932, 6, 429–440.

lar to the great drowsiness (schlaftrunkenheit) reported and exhibited by Dahl's *S*s at these intervals. In our case, however, *G* did not exhibit this variation in performance as did *V*, and yet the results of the two are the same for the various time intervals.

One might readily expect some divergence in the results of these three investigations due to the different methods used. Undoubtedly the procedure of our study was the most difficult of the three memory tasks and would seem to require much more thorough cooperation from an aroused *S*. In view of the great divergence in procedure, and in difficulty of method, the fact that the three studies agree in confirming the conclusion that retention after 4 and 8 hr. of sleep is definitely better than after 4 and 8 hr. of waking, would seem to be of major significance. For reasons stated in the procedure our study would seem to be the most reliable and exacting of the three. It is evident, however, that more investigation of the 1- and 2-hr. intervals is desirable.

TABLE VIII
PERCENTAGES RETAINED AFTER CERTAIN TIME INTERVALS ACCORDING TO VARIOUS INVESTIGATORS USING SAVINGS METHOD

Time in hrs.	*Ebbinghaus Obtained-Theoretical-Diff.*			*Radosavljevich*	*Finkenbinder*	*Luh*	*Boreas*	*van Ormer*
1 hr.	44.2	46.7	–2.5	70.7	72.8	65.9	64.19	44.1
2 hrs.	....	....	....	....	69.4	....	....	38.5
4 "	....	....	....	....	66.4	54.9	....	34.1
8–9 "	35.8 (8.8 hrs.)	34.5	+1.3	47.4 (8 hrs.)	65.5 (8 hrs.)	....	57.59 (9 hrs.)	23.9
16 "	....	....	....	....	63.0	....	....	....
24 "	33.7	30.4	+3.3	68.9	57.8	52.1	59.21	....
36 "	....	....	....	....	58.8	....	....	....
48 "	27.8	28.1	–0.3	60.9	55.5	47.7	50.40	....

Table VIII offers a comparison of our waking curve with other curves of retention. As far as it goes, our curve is more comparable to that of Ebbinghaus than to any of the others. Notice that there is perfect agreement at 1 hr. Our 8-hr. retention, however, is definitely lower than his. The various exacting features of our procedure would seem to explain the lower percentages of retention than other investigators, following Ebbinghaus, have obtained. It would seem that the lists constructed by us and the learning procedure, especially the elimination of rhythm, have made our investigation a closer approximation to a study of pure rote memory than was even that of Ebbinghaus. This may account for our percentage of retention definitely lower than Ebbinghaus at 8 hr. For

it would seem that rhythm used in learning and meanings interjecting themselves might be more of an aid to retention after 8 hr. than over a shorter period of time.

Summary of Results

1. No reliable difference was found for the average number of repetitions required for the learning of the 1st, 2nd, and 3rd lists of an experimental sitting.

2. The practice curves demonstrate the great amount of improvement which can take place from continued practice in a particular function; they further show that practice was not eliminated from the main experiment—but the data seem to indicate that it was rather well controlled by the planned haphazard orders of the experiments—; in addition the curves show an interesting difference in the learning performance of the two *S*s.

3. In studying diurnal variation in learning performance, reliable differences were found between 9 : 30 A. M. and 11 : 30 P. M. Partially reliable differences were found between 9 : 30 A. M. and the 1-, 2-, and 4-hr. night checks. No day time variation in *G*'s learning performance was found for the day time periods tested. The 10 : 30 and 11 : 30 A. M. and the 1 : 30 P. M. periods for *V* show partially reliable differences from the 9 : 30 A. M. period.—For the direction and amount of these differences see Table IV, p. 32.—The argument put forth by various investigators that fatigue would cause learnings at 5 : 30 P. M. to take longer is not supported by our results. There are probably definite individual differences in diurnal variation of ability to learn nonsense syllables.

4. In the retention curves of the 1st, 2nd, and 3rd lists there are various irregularities which are statistically unreliable, the data not making it possible to make any conclusions regarding the comparative retention of the 1st, 2nd, and 3rd lists of an experiment. Accordingly, for the comparison of sleep and waking, the retention was calculated on the basis of the three lists combined.

5. In the case of our *S*s retention of nonsense syllables as measured by the savings method is definitely better after 8 hours of sleep than after the same time interval of waking; there is a probably reliable difference in favor of sleep at the 4-hr. interval; there is merely a suggestive difference in favor of sleep at the 2-hr. interval; and there is no difference between sleep and waking at the 1-hr. interval.

6. The similarity of the curves of the 2 *S*s is significant in view of the differences in the attitudes with which they came to the investigation.

7. The fluctuations in the sleep curves after the 1-hr. period are unreliable.

8. The method of learning employed by the *S*s was largely a visualizing procedure.

9. The results of this investigation stand mid-way between those of Jenkins and Dallenbach and of Dahl, on the question of the difference between sleep and waking at the 1- and 2-hr. periods.

10. The waking curves of retention, as far as they go, approximate more closely those of Ebbinghaus than of any other investigator.

PROBLEMS FOR FURTHER STUDY

The following additional problems are suggested in connection with this question of sleep and retention.

1. Comparison of the sleep and waking curves from 8 hr. on. Heine's study was related to this.

2. A comparison of retention over 24 hr., when the sleep comes after interpolated waking intervals of different temporal length.

3. A comparison of sleep and waking with sleep occurring in the day time and waking at night.

4. A comparison of retention after sleep and waking with attempts to sleep more than 8 hr.

5. The study of retention when the subject is anesthetized immediately upon completion of the learning.

6. Further study of the 1- and 2-hr. periods.

7. Repetition of Dahl's study taking especial care to have the *S*s thoroughly awake to ascertain if recognition is really poorer after 1- and 2-hr. of sleep than after waking.

8. A study of muscular tension in relation to this problem of sleep and retention.

9. More studies of retention when the learning and relearning are done in the hypnotic state; and also when the learning is done in the normal state and the recalling in the hypnotic state.

10. More studies such as Spight's, using spaced learning and comparing rest periods filled with sleep, *versus* rest periods of waking.

11. Studies of sleep and retention using meaningful material of various sorts and motor performances.

THEORETICAL CONSIDERATIONS

Heine (12) interprets her results in favor of sleep as due to the elimination of the retroactive inhibition produced by the day activities which normally follow learning. Jenkins and Dallenbach conclude that, "The results of our study as a whole indicate that forgetting is not so much a matter of the decay of old impressions and associations as it is a matter of interference, inhibition, or obliteration of the old by the new." (14, p. 612). Dahl (5), stating that retroactive inhibition has never been demonstrated for recognition,[38] rejects Heine's and Jenkins and Dallenbach's conclusion and merely suggests that the fact that sleep favors recognition must be due to some influence which either sleep or waking has upon the impressed material.

It is suggested that our results in favor of sleep are brought about by the absence of the inhibition and obliteration of the learned material by the waking activity. But if it is just a question of absence of inhibition, why do the curves of sleep and waking coincide at 1 hr. and diverge from then on? May it not be that the waking activity not only inhibits and obliterates what has been learned, but that it also prevents or holds in check a perseveration or consolidation process which continues for a while in the nervous system after the impression of the learned material?[39] The perseveration process may frequently be at its height for the first part of the hour following learning.[40] The inhibition produced by the waking activity for the first ten minutes after learning, before it is possible to get to sleep, and possible inhibition from the process of going to sleep, itself, thus interfere with a very effective part of the perseveration process; and accordingly their effects practically overbalance, for the 1-hr. sleep period, the 50 minutes of sleep within which there is a possible continuation of the perseveration process and the absence of inhibition and obliteration by waking activity. It is also very possible that the process of getting awake and the little activity interpolated between awaking and relearning also have an inhibitory effect which would aid in keeping the retention

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38 Dahl probably based his statement on the work of Heine (12). Woodworth and Poffenberger (32, p. 112–113) criticize Heine's procedure and point out that certain work by Strong (30) demonstrates retroactive inhibition for recognition.

39 This idea of a perseveration process is not new. See *e.g.* Müller and Pilzecker (21) and Skaggs (28), who have argued for a perseveration process to explain retroactive inhibition. Woodworth (31) has also argued for a consolidation process following active learning.

40 Skaggs (28, p. 27), "Work introduced immediately after the learning produced the greatest amount of inhibition." Also see Skaggs (27).

for the 1-hr. sleep period down to the level of the waking period. These results on the one-hour period would suggest that a perseveration process does exist, at least for the first 10 or 15 minutes following learning.

The results in favor of sleep for the remaining time intervals, and the results of the Jenkins and Dallenbach study from 2 hr. on, are hard to reconcile with the law of atrophy through disuse. It may be argued that sleep refreshes the organism and recall is thereby benefited. But if such is the case it may be asked why retention, or reinstatement, after 8 hr. of sleep is no better than after 1 hr. of sleep? Certainly refreshment or physiological recovery—if produced by sleep—is far from complete after 1 hr. And it must be remembered that the retention after 1 hr. is the same for sleep and waking in the case of our *S*s, which would hardly be the case if the sleep had produced a refreshed state which was beneficial to reinstatement. The more plausible explanation of our results in favor of sleep for the latter three periods would seem to lie in the further absence of inhibition and obliteration of the learned material by the waking activity; with the additional possibility that the perseveration process may continue for a longer period than the first hour.[41]

In view of the present state of neurological knowledge concerning learning, any discussion of the nature of the neural activity which may produce this perseveration of learned material and also bring about this inhibition and obliteration of learned material by waking activity, would seem rather futile. Our results, however, would suggest that a primary factor in forgetting is the action of the interpolated activity as it inhibits a consolidation or perseveration process and produces inhibition and obliteration of the learned material.

CONCLUSIONS

(1) For our *S*s the retention curves of nonsense syllables for periods of sleep and of waking coincide at 1 hr.; from then on the sleep curve is practically a horizontal line up to 8 hr., with the waking curve falling away from the former after the 1 hr. interval at an angle of from 20 to 30 degrees, slowing off appreciably after the 2-hr. interval, but failing to show any appreciable slowing off at the 4-hr. interval.

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[41] Note in this connection the tendency of interrupted activities to perseverate. See *e.g.* Zeigarnik (33). It is also relevant to note the tendency of the day's activities to perseverate some hours later, or as one lies in bed before going to sleep. Woodworth (31, p. 92) has suggested the possibility of a consolidating process lasting for some *hours* following active learning.

(2) In consideration of the studies of Heine, Jenkins and Dallenbach, Dahl, and of our own results, the conclusion seems established that the usual amount of daily sleep (about 8 hr.) favors retention of nonsense syllables over that time interval.

(3) Regarding the more specialized generalization that retention of nonsense syllables is better *even after short time intervals of sleep* than it is after the same intervals of waking, the results are conflicting. Jenkins and Dallenbach's, Dahl's, and our own results warrant the conclusion that retention of nonsense syllables is better after 4 hr. of sleep than it is after 4 hr. of waking. The former of the two above mentioned studies and our own would suggest that the advantage of sleep is not as great at 4 as at 8 hr.

(4) In view of our results it is suggested, with Jenkins and Dallenbach, that the favoring of retention by sleep accounts for Ebbinghaus' discrepancy from 8.8 to 24 hr. Sleep would also seem to be a partial explanation for the discrepancies in the curves of Radosavljevich and of Boreas from 8–9 to 24 hr. These investigators, not controlling diurnal variation in performance, probably have it as at least one other factor to explain their discrepancies.

(5) The fundamental similarity between the learning of nonsense syllables and of other verbal material seems to warrant the extension of our conclusions to apply to other verbal material. Accordingly, the four above mentioned studies, combined with Spight's suggestive results, indicate that retention of any verbal material is probably better after 4 or 8 hr. of sleep, than after the same time intervals of waking.

(6) Accepting conclusion (5), the results of these studies on sleep and retention support a new principle of efficient study. Assuming that there is little or no decrease in learning efficiency at the late evening hours, the advantage of night study, as suggested by Jenkins and Dallenbach, becomes evident. "Little is forgotten during sleep, and on waking, the learner may take up the task refreshed and with renewed vigor." (14, p. 611).

(7) Our results from the 1-hr. period suggest the existence of a perseveration process for at least 10 to 15 minutes immediately following learning.

(8) The results of these studies on sleep and retention, combined with the recent work on retroactive inhibition, necessitate the revision of the law of disuse. As suggested in a similar way by other writers,[42] it is quite possible that forgetting is a func-

[42] Foucault (9); Jenkins and Dallenbach (14); Hunter (13); Robinson (25); McGeoch (18) and (19).

tion of the nature of the interpolated experience, the altered environmental contexts, and the organic state of the individual during and subsequent to learning, rather than intrinsically a function of the period of disuse.

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